n contemplating the aspects of nature," and "derives enjoyment from studying the forms, habits, and relationships of animals and plants," but how can he do so, and thus become a "biologist," unless he peers "through the tube of a compound microscope," etc., and does his proper hardening, and staining, and "monographs the same bit of tissue." How such investigations can "obscure the objects" we are trying to explain is rather a mystery. If, at least, anybody allows them to obscure our general views, there can be no speaking of scientific work. Natural history has become, in our century, so broad that no man possibly can become a "general naturalist" or a good "faunal naturalist" any more; he will, at least, not be able to treat all the questions that arise in any other way but in that of the amateur. The objects of our investigations lie a little deeper than to glance at all that is "most beautiful" and attractive to the eye.

How the article comes to the conclusion that the study of the minute structure is histology or that of development embryology, is rather doubtful. Further, I am anxious to know if any of the readers walking over the scientific border-land commanded by the naturalist who might be educated according to the principles given in the article of which we speak did ever meet with "the various pathogenic micrococci of fermentation and disease" which are mentioned (p. 353). However, I shall not enter upon further details, but turn towards the view expressed in the said article about "section-cutters and physiologists," and I shall try to show that the work done by the workers in this particular field is far from being one-sided, at least, when we are speaking of real scientific men who put an equally fair valuation on all of the branches of their science. There are, as Professor E L. Greene said, "a good many men trying to figure somewhere" as scientific writers, but where are the scientific men to be found when we look towards the "scientific border-land" (Greene)? Therefore, we shall see that the right sort of scientific physiologists do not dare to depreciate any of the branches of their science.

Professor P. L. Panum once said that he who would not acknowledge physiology as the fundament of pathology and of the other departments of medical science has no right to be called a scientist. The vegetable physiologist who does not know anything about the principles of agriculture, horticulture, and forestry also loses this right, and so he does, if he is ignorant with regard to a great deal of the practical, industrial branches. If we go to the opposite side, he must know how to carry out more minute investigations; he cannot avoid being something of a "slice-cutter," and if he should be unfortunate enough to find "some new form of cell or new property of protoplasm," he must understand how to trace such a discovery as far as it can be traced. I am, therefore, very much surprised to hear that "the modern school of histologists, under the head of biology, teach little besides the minute structure and function of tissues." For my personal account, I have studied physiology almost from the time when I could appreciate the blessings of the study of natural history, but I have never met a man who claimed to be a physiologist, — *in casu* vegetable physiologist,— and who, speaking, for example, of the nitrogen question, did not know the theoretical investigations quite as well as the practical experiments with fertilizers. But it must be noted that natural science has, at present, reached such an extent that no man possibly can cover the whole ground. Thus we have, with regard to special work, to become specialists, and, therefore, it is possible to take a farmer's boy and make out of him "a general naturalist of the present day" or a "local faunal" — or floral — "naturalist." He will be no scientific man.

"Biological" teaching is a failure for other reasons than those presented in the article. A college professor may offer a course in "general biology" and include "cell structure and the structure of the less complex tissues of animals and plants." But this is not "general biology;" the structure of two different forms has not the least to do with biology, it comes under the heading of internal or external morphology, and, when making a study of this kind, the student does not see more of life in general and of the laws by which it is governed than he saw before. Here the experimental physiology of animals and plants must be held up before a school of "biologists" who are following a phantom of their own imagination if they really believe that function can be explained out of form. It is here that "the pendulum has swung too far," and it is not in the direction of "exclusive microscopic and physiologic work." The latter is safe enough. The fault lies entirely in the methods of modern biology, which begins with giving itself a wrong definition. If the modern biologist had cared more for experimental physiology, he would know now how to direct his actions when the pendulum "swings back."

If I understand the article aright, the student should begin his biological work with elementary "general biology." He will, then, come to the university without, practically speaking, knowing anything about "biological" questions, and he will plunge into the study of cell-structure at once. This beginning of a course would be anything but beneficial to the young, ignorant student. If we take the example of the farmer's boy, he would naturally have to start with the study of what we call external morphology, collect plants, insects, or shells, and perhaps study their ways. It would be entirely lost on him to train him in the study of the cell and its organs. The other special sides of biology which are proposed for study are: 2. Morphology, taxonomy, and relationships; 3. systematic work in widely-separated groups; 4. faunal work; 5. the distribution of life in time and space; 6. the principles and philosophy of biology.

These are the constituents of "biology!"

If it were so, the condition of natural science would be very lamentable. Not a single word or hint is given about the existence of experimental work, which should be the main factor in the whole course of training. It is true, as has been said, that "sham" is a hard expression, but here it might be used very properly. Many of the "biologists" of the present day will hardly understood my view, because they have been taught to regard the study of morphology as the essential part of their biological studies, but the physiologists will do so, because they know that we can take but very few steps in any direction without experiment. So long as biological courses do not include a proper course in experimental physiology of animals and plants, they cannot be called properly scientific. Anybody who will not believe this may be referred to Paul Bert's "La Science Experimentale."

There is no danger that I should have misunderstood the article. I see clearly that it wishes the "systematic biology," which might have been called, more logically, biological classification, to take a place a little more ahead of what it holds at present. But, trying to give a fair valuation of the other branches of physiology, it fails entirely. It is well known how language can command the thoughts, and if biologists go forth without knowing what they are teaching, the present confusion will grow instead of being settled. Perhaps "biology" will gain more and more lovers and become (as it is) very fashionable, but the amount of restless work, chasing new problems and pursuing all that is interesting merely because it is new, will not, in time, be very much valued. Nothing can save 'biology" except experimental physiology.

J. CHRISTIAN BAY.

Missouri Botanical Garden, July 7.

Mr. McGee and the Washington Symposium

It strikes me as curious, and certainly contrary to scientific usage, that the succinct statements made by Mr. King as to the limitations of his inferences on the earth's age are ignored by our Washington friends. One might actually imagine that we were not on the scent of polymerism [1] considered either with reference to its volume or the inseparable thermal effect; or that we were unaware of the high pressure and long range thermal variations of the physical constants of rocks. It takes so little time, so little cerebration to adduce critical commonplaces of this nature,

[1] If there was one subject in which we imagined that our work had reached the point of prolixity, it was the change of chemical or molecular constitution as resulting from temperature and stress. (Cf. Am. Journ., xxxiii., p. 28, 1887; ibid., xxxvii., pp. 339, 351, 1889; ibid., xlii., p. 498, 1891; ibid., xliv., p. 242, 1892; etc.; Phil. Mag., xxxi., p. 9, et. req., particularly § 25, 1891; ibid., xxxv., p. 174, § 3, 1893; Am. Chem. Journal, xii, p 1, 1890; Bull. U. S. Geolog. Survey, No. 94, 1892; and elsewhere). And now comes Mr. McGee with obviously well-meant instruction on the feasibility of our polymeric mechanism.

that they are always bountifully forthcoming. But the things which one really wants, the physical character of an alleged discrepancy, its numerical value, the so-many per cent of error under such conditions,— these one is left to wish for in vain, supposing that one has not long since learned to pay the personal groaning for the personal satisfaction. So far as I am concerned, if I could not adequately state how big a sin it is under which somebody else is staggering, I should prefer to hold my peace, believing that matters of vague conjecture are not fit to be chronicled. Nobody on the same side of common sense would today attempt to exhaust so complex a problem as the one in question in a single instance. It is reasonable, however, to try to remove piece by piece, element by element. What we did was an endeavor to remove the preponderating element, and I must reiterate that if our respite had not been cut short by recent unfavorable legislation, other things would have been brought out in their turn and in due time. Perhaps it is heresy to state that an immense future awaits laboratory research in physical geology; but stating it, one would like to refer not so much to the punching of clay or the pulling of taffy candy, as to legitimate physical measurement However, others have survived even the odium of cultivating "exact" methods. We are soothing ourselves with the comfort of so thinking.

CARL BARUS.

Phys. Laboratory, U. S. Weather Bureau, Washington, D. C.

The Lac de Marbre Trout, A New Species.

DESCRIPTION: B. 11 12; D. 13; A. 13; V. 9; P. 14; Vertebræ 60.

The specimen described is about twelve inches in length. Body subfusiform, compressed, pointed at snout, slender at the tail. Height of body near one-sixth of the total length; head one-fifth, crown convex. Snout one and one third, and interorbital space one and one-half times the eye. Eye little less than one-fifth of the head, two-thirds of the space between the orbits on the forehead. Mouth large; maxillary straight, extending backward almost as far as the hinder edge of the eye, bearing strong teeth on its lower edge for nearly its entire length. Teeth on intermaxillary and mandibles stronger. The tongue bears a series of four strong hooked teeth at each side, and behind the glossohyal on the basibranchials there is a band of several series of smaller ones. Gill rakers straight, short, sharp, rough, 8 + 14 on the first arch. Opercle thin, with a few striæ. Scales very small; apparently there are about two hundred and thirty in the series immediately above the lateral line and more than two hundred and fifty in a row five or six scales above this. Distance from first ray of dorsal to end of snout little more than that from the same ray to the tip of the adipose fin. The middle of the total length falls halfway between the ends of the hinder rays of the dorsal and its base. Dorsal and anal fins are slightly emarginate at the ends of their median rays. Pectorals and ventrals small; base of latter slightly behind the middle of that of the dorsal. Caudal pedicel slender, notch very deep, hinder border sinuous, as in Salmo alpinus, lobes pointed. The caudal notch is deeper in this species than in any other of the American forms except S. namaycush.

Back dark brown with an iridescent blueish tint, unspotted. Dorsal dark, clouded, without spots or bands. Pectorals, anal and ventrals orange in the middle, yellowish or whitish toward bases and at their margins. The dark color of the back shades into whitish tinged with pink below the lateral line. Ventral surface white, no doubt reddish in breeding season. Head black on top, silvery on the cheeks, white beneath. Flesh pink. Caudal fin yellowish toward the base, brown toward the hinder border, which has a narrow edging of light color. Faint areas of lighter tint suggest a few spots of red in life along the lateral line; the condition of the specimens is such that this may be left in question, as also the number of caeca or presence of parr-bands of which there are faint indications

This fish is evidently allied to the blue-back of the Rangeley Lakes, S. oquassa, but reaches a greater size than that species, and is readily distinguished by the maxillary and its dentition, the caudal fin, and the coloration. Similarly when compared with S. arcturus, S. stagnalis and S. Rossi, it is seen to be quite distinct. With the saibling, S. alpinus, introduced in Sunapee Lake and elsewhere, it has still less in common.

Our specimens were taken in Lac de Marbre, Ottawa County, Province of Quebec, Canada, whence they were sent by favor of the Hon. J. G. A. Creighton. They reached us at the instance of Mr. A. N. Cheney, fishing editor of *Shooting and Fishing*, who when asked to suggest a specific name replied with the question, "How would it do to name it for Mr. R. B. Marston, editor of *Fishing Gazette*, London, an Englishman overflowing with good feeling for everything pertaining to fish, fishing and America, and who is doing much to enhance friendly interest between the people of the two countries?" In consequence of the suggestion this handsome char, one of the handsomest of our species, is introduced under the name, *Salmo* (*Salvelinus*) *Marstoni*.

S. GARMAN.

Mus. Comp. Zool., Cambridge, Mass.

Tucumcari.

THE writer first visited this historic locality in 1887, before he had had opportunity to define the Denison beds at the top of his Lower Cretaceous section in northern Texas, and fell into the error, which others have not escaped, of concluding, from the peculiar Jurassic-like *Gryphæa dilatata*, Marcou, the only fossils found upon that visit, that the beds were Jurassic, and so published his opinion.

Later, however, after having had an opportunity to complete his study and arrangement of the stratigraphy of the Comanche series in central Texas, he discovered in the Denison beds[1] of his Washita Division certain features which led him to believe that his early diagnosis of the Tucumcari beds was erroneous, and that they were really closely allied in age to the Denison beds. Under this impression, which was communicated orally to all interested, he availed himself of the first opportunity to revisit Tucumcari, April 30, 1891. He then discovered in association with *G. dilatata* the list of additional species herewith given, and, at earliest opportunity, under date of May, 1892, published, in a general discussion of the region, the following revision of his previous conclusions, which was the first printed announcement of the Cretaceous age of the *G. dilatata* beds: —[2]

"The Trinity Sands and Red Bed Regions.

"The writer has twice visited the Mesa Tucumcari and found it a most interesting geological remnant of the former area of the Llano Estacado The table or summit described by Capt. Simpson is covered with the typical Llano Estacado formation, identical in composition and formerly continuous with the sheet which covers the Llano proper, some 20 miles distant. Below this is a vertical escarpment of 50 feet or more of typical Dakota sandstone resting upon loose sands and clays, forming a slope identical in aspect and fossil remains with the Denison beds of the Washita Division, which have been eroded away from the 400 miles intervening between it and the main body of those beds at Denison, Texas. Beneath this is a large deposit of the typical Trinity sands *country*[3] of white pack sands, thin clay seams and flagstones, while the base is composed of the typical vermilion sandy clays of the Red Beds."

Notwithstanding the above clear statement of my opinions, the Third Annual Report of the Geological Survey, printed nearly a half-year afterward, devotes many pages to asserting that I held to the Jurassic age of the *O. dilatata* beds at Tucumcari. Upon pointing out this misquotation, instead of acknowledging the error, and repairing the injustice, it was followed up by a privately

[1] Denison beds as originally defined and used by writer. Not the Denison beds of Taff, as used in an entirely different meaning. Compare Bulletin of Geological Society of America, Vol. II., p. 591, and Third Annual Report of Texas State Geological Survey.

[2] "On the Occurrence of Artesian and Other Underground Waters in Texas, Eastern New Mexico, and Indian Territory West of the 97th Meridian," by Robert Thomas Hill (being part of Vol. III of Senate Document 41, 1st Session, 52d Congress, Washington, May, 1892

[3] For "country of" read "consisting of" — a typographic error.